The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Mark C. Marvin−DiPasquale

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0268: The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Funding:

Do not fund

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Processes Controlling Delta Water Quality

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

Very high. Understanding the cycling of mercury in restored wetlands within the Bay−Delta system and the mechanisms driving some of the complex chemistry involved in mercury mobilization in wetlands is of high strategic importance because of the implications for wildlife and the restoration activities underway in the Bay−Delta system.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

The budget is high (1.8M+) but the research team required to address this complex question is multi−institutional and multidisciplinary.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

This proposal addresses a critical issue looming over wetland restoration efforts in the Bay–Delta system. The research team is experienced and knowledgeable on issues related to mercury cycling in wetlands as evidence by ongoing research projects related to the topic. However the proposal has a number of shortcomings that limit it's utility for management. The hypotheses stated in the proposal are simplistic and lack the level of detail and sophistication of the proposed scientific effort. They cite work by others that already affirm all three of their stated hypotheses. For example, they know that wetland plants affect mercury methylation (H1), nutrient stimulate plant growth and thus mercury cycling (H2), and mercury cycling varies between seasons, species and stages of development (H3) as per citations within the proposal. More important are the limitation of the experimental design. The experimental design should be reconsidered in light of reviewer comments. Limitations of the experimental design include the lack of replication of the nutrient addition experiments and the lack of blocking of important factors that could affect the outcome and interpretation of their results. These shortcomings are likely to limit the applicability of the results in a management sense if the study cannot be revised to address these issues. On the one hand it is important to fully understand the details of mechanisms underlying what is a very complex set of factors driving mercury cycling and mobilization in restored wetlands. However, given the ongoing restoration efforts and the critical needs for a comprehensive understanding of marsh restoration actions, a study that addresses a broader set of factors with less detail may be more useful and practical. At this point in time it may be most important for managers to have the knowledge to fully understand if and how much all the critical factors influence the rate of mercury mobilization (including marsh elevation and flooding frequency).

Selection Panel (Discussion) Review

fund this amount: $**0**
note:
do not fund

The panel emphasized the importance of this topic of mercury for restoration, and was disappointed that this proposal was not focused more directly on management−relevant questions. This is a well−respected group of researchers, proposing sophisticated chemistry. Given their expertise in this topic, they should have been able to propose more sophisticated hypotheses addressing mercury cycling. If the question is whether to restore or not to restore these particular habitat types, this research will not answer that basic question.

The panel echoed and amplified a number of the concerns of the technical synthesis panel. The panel felt that the biggest problems are the proposed experimental design, and the inability of the study to address the fundamental issue of restoring or not restoring wetlands because of enhanced mercury flux. They propose a BACI analysis, but their methodology does not support this analysis. The panel agreed that this is the logical, strong group to do this work. However, as proposed, the panel agreed that it would not deliver what is necessary to address the key management issues.

Panel Ranking: Do not fund.

Technical Synthesis Panel Review

Proposal Title

#0268: The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Final Panel Rating
above average

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This proposal addresses a very critical issue, both for CALFED as well as Baylands and South Bay Salt Pond restorations. Uncertainty about how mercury methylation will be affected by various restoration scenarios is perhaps the most important unknown in a huge amount of imminent restoration in the greater San Francisco Bay region and Delta. The work proposed is very thorough and critical at a small scale, with great analytical virtuosity that is obviously appreciated by some reviewers. From the standpoint of actual restoration, the proposal is somewhat deficient in ecological perspective – it ignores effects of elevation/flooding frequency on redox conditions in soils, which may be a much greater determinant of mercury methylation rates than vegetation or nutrient levels. As one reviewer notes, "The proposed sampling scheme seems to assume that each wetland site is a homogeneous unit," with no need to stratify treatments by elevation. Redox conditions in wetland soils are notoriously patchy in space – often ä34S values in wetland soils, which reflect sulfur reduction processes, are so wildly variable that few inferences can be drawn from them. Although the work proposed is excellent in considering oxygen gradients relative to plant roots, such gradients may vary with flooding regime even for the same plant species. Such variations necessitate (1) higher

sample sizes and (2) careful blocking of factors that affect redox conditions in the experimental design. Ignoring spatial variations in elevation/flooding frequency, and the unacceptably low replication for different treatments, might greatly limit the generality of the results of this project, and at least would cast strong doubt on the generality of the results. Nutrient amendments, which might be important in management, are mentioned on p. 16 and in Fig. 9, but are never really explained or justified. One reviewer noted that allocating a third of the budget to nutrient−addition experiments may be unjustified, relative to effects of hydrology and resulting redox conditions on Hg cycling. There is no replication at all of the nutrient−addition plots, which will make it very difficult to determine if any changes observed resulted from nutrient additions. The above points emphasize a common tradeoff between making many refined measurements on a few samples, versus making fewer measurements on more samples. Although the analytical approaches are very thorough, the expected importance of various pathways of mercury methylation and flux are not prioritized – it seems likely that some pathways might be relatively minor. If so, fewer measurements on a larger number of plots that incorporate hydrology into the experimental design might yield more useful results from a restoration perspective. Despite the above shortcomings, the need for mechanistic knowledge of factors affecting rates of mercury methylation is very great. Much improved understanding of certain aspects will result from this study, even if they might not be the most important aspects. The track records of the PIs are quite good, and much important work will be done. Most reviewers considered the budget reasonable. Again, prioritization of the expected importance of different pathways of mercury flux might allow reallocation and perhaps reduction of some aspects of the work. It is difficult to judge these priorities based on this proposal. Much equipment will be needed for this work and no equipment purchases are budgeted, which makes the proposal quite cost−effective in that regard.

Additional Comments:

This proposal addresses a very critical issue, both for CALFED as well as Baylands and South Bay Salt Pond restorations. Uncertainty about how mercury methylation will be affected by various restoration scenarios is perhaps the most important unknown in a huge amount of imminent restoration in the greater San Francisco Bay region and Delta. The work proposed is very thorough and critical at a small scale, with great analytical virtuosity that is obviously appreciated by some reviewers. From the standpoint of actual restoration, the proposal is somewhat deficient in ecological perspective – it ignores effects of elevation/flooding frequency on redox conditions in soils, which may be a much greater determinant of mercury methylation rates than vegetation or nutrient levels. As one reviewer notes, "The proposed sampling scheme seems to assume that each wetland site is a homogeneous unit," with no need to stratify treatments by elevation. Redox conditions in wetland soils are notoriously patchy in space – often ä34S values in wetland soils, which reflect sulfur reduction processes, are so wildly variable that few inferences can be drawn from them. Although the work proposed is excellent in considering oxygen gradients relative to plant roots, such gradients may vary with flooding regime even for the same plant species. Such variations necessitate (1) higher sample sizes and (2) careful blocking of factors that affect redox conditions in the experimental design. Ignoring spatial variations in elevation/flooding frequency, and the unacceptably low replication for different treatments, might greatly limit the generality of the results of this project, and at least would cast strong doubt on the generality of the results. Nutrient amendments, which might be important in management, are mentioned on p. 16 and in Fig. 9, but are never really explained or justified. One reviewer noted that allocating a third of the budget to nutrient−addition experiments may be unjustified, relative to effects of hydrology and resulting redox conditions on Hg cycling. There is no replication at all of the nutrient−addition plots, which will make it very difficult to determine if any changes observed resulted from nutrient additions. The above points

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

The panel agreed that the proposed work is analytically sophisticated and the general topic is critically important to marsh restoration.

A major problem the panelists identified is that the proposed work ignores effects of elevation/inundation frequency on redox conditions and soils. The panel considered these to be potentially very important. High heterogeneity in the soil redox conditions would be expected. These factors would

necessitate blocking by elevation and greater sample size. The sample size is small as proposed, and this study would need more replication.

The panel identified the lack of replication of nutrient amendments as an important problem. In general, if fewer measurements were planned on a larger number of plots, this study would be more generally applicable.

The panel considered that the researchers would look at a large number of factors in a few replicates, and that this would provide much information on a small scale, but in the end this would not outweigh the disadvantage of low replication.

The principal investigators have a good publication track record, and have great analytical capabilities.

The budget was well justified.

The panel found that a surprisingly large amount of attention was given to an operationally–defined methodology for determining the reactive solid phase of mercury, the "reactive" form quantified from acid leaching. Another concern was the lack of experience in Hg flux estimates for volatile Hg.

Rating: above average

Technical Review #1

proposal title: The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals, objectives, and hypotheses are clearly stated and consistent. The overall goal of understanding how marsh restoration affects Hg cycling seems important. It should be made clear, however, that what they are really looking at is how Hg cycling is affected by: the presence of vegetation; the type of vegetation (Spartina foliosa vs. Salicornia virginica); and nutrient loading. One question I have (as a scientist from the East Coast): does restoration in SFB generally mean replacement of non−vegetated systems (e.g., salt pond) with vegetated systems (salt marsh)? If the answer is "no," the focus of this proposal on the role of vegetation may be misplaced. If the answer is "yes," I still wonder about whether the systems they are studying are a good model for the difference between non−restored and restored sites (more on this below).**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The authors have a very good handle on the existing**

	state of knowledge of nutrient and Hg cycling in salt marshes and the role of the microbial community and the rhizosphere. The conceptual model is clearly stated and builds on previous work.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The project is generally well-designed. The proposal is well-organized and the tasks are clearly stated and related directly to the objectives. However, I have several issues with the approach: § Their study aims to assess the role of vegetation (presence/absence), vegetation type, and nutrient loading in Hg cycling by comparing plots with different vegetation and nutrient regimes. This is a worthwhile goal in itself. However, as mentioned above, it is not clear to me that this captures the role that restoration plays in Hg cycling (which is how they frame the project goals). There are many differences between restored and unrestored sites besides the presence or absence of vegetation, especially differences in hydrology and redox potential. Their study design does not capture these differences, but rather focuses on differences within restored and natural marshes between vegetated plots and "control" plots that experience the same hydrologic regime, but are kept plant-free by clipping. To truly assess the role of restoration, they need to look at restored vs. unrestored sites. § They claim to be using a BACI (before-after-control-impact) design. However, the logic of BACI requires monitoring all plots before the impact in order to assess pre-existing differences between the plots; this allows interpretation of differences between the plots after the impact. In**

this case, that would mean sampling all plots in the first year, then introducing both the nutrient amendments and the plant clipping treatments in the second year. Instead, they sample the vegetated and plant−free plots in the first year, and begin the nutrient amendments (on different plots) in the second year. This is not a BACI design, and it is not clear what advantage this phased approach offers, other than allowing them to determine the amount of nutrient addition based on the first year's data – but surely they can determine a rough amount based on existing data or preliminary analyses. § The plot design for the nutrient amendments seems like a mistake to me. First of all, plot SAA is essentially the same as plot SVA, only larger. Second, they have no replication at all of the nutrient plots, which will make it impossible to determine if the changes they are seeing are a result of nutrient addition. Third, why the larger plots? Perhaps it is to provide replication, but if so, it is pseudo−replication. They need 3 independent 1m2 plots (to which nutrients are added independently), not 1 3m2 plot! § Much of the research effort (roughly a third of the budget) is focused on nutrient cycling in the marsh, including measurements of N and C stable isotopes, denitrification, N−fixation, exoenzyme measurements, and sediment incubations. I have mixed feelings about this. I strongly believe that this effort will provide interesting and valuable results about the impacts of nutrients on marsh processes. At the same time, I am not sure that this level of effort on the nutrient front is neccesary to understand Hg cycling, and in particular to address their hypothesis 2 (nutrients stimulate plant growth and thus influence the rates of key plant−Hg interactions). § The controls on Hg cycling are quite complex, as reflected both in their literature review and in the data that they present. This is also reflected in the non−directional nature of their hypotheses: they predict that the presence of wetland plants will affect the concentration of bioavailable Hg, without predicting whether it will

	increase or decrease. Given that plants can have multiple, possibly counteracting effects on Hg cycling, I worry a little that their data may not be interpretable in terms of a relationship between plants and Hg availability. There may just be too much spatial and temporal variability in parameters like hydroperiod, rooting depth, and plant physiology. For example, Figure 5 seems to indicate that seasonal variability in Hg cycling is just as important as whether a site is vegetated or not. With 3 sampling dates per year, can they capture enough of this temporal variability to make predictions about the effect of re−vegetation on overall bioavailable Hg?
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Their tasks are well−documented in Table 2, which is very useful. The methods are all quite reasonable and are familiar to the authors. My only complaint would be that the measurement of denitrification with the acetylene block is now considered by many to be an invalid approach.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**N/A**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Valuable products are likely, particularly an understanding of the effects of vegetation and nutrient loading on Hg and nutrient cycling.**
Rating	**very good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**These scientists are top researchers in their field and are highly qualified to carry out this project.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is reasonable and is reasonably distributed among the different entities**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I generally think this will be good research, though I have some qualms about: the sample design; the degree to which their results will be relevant to understanding the effects of restoration; and the degree to which they will be able to draw simple relationships between vegetated status and Hg cycling (see Approach section).**
Rating	**very good**

Technical Review #2

proposal title: The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**In turn;** **Are the goals, objectives and hypotheses clearly stated and internally consistent? – The goals are clearly stated on p. 3 and the hypotheses are clearly stated on p. 7–8. The goals and hypotheses presented are internally consistent.** **Is the idea timely and important? – Absolutely, yes. The CALFED goals of both increasing wetland acreage while also minimizing MeHg production require that research such as this be supported. So, the importance of this work to this program is manifest. Beyond that, the absence of comprehensive work of this type in the scientific literature makes clear that this work is timely and important at a scale beyond the purview of CALFED.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**In turn;** **Is the study justified relative to existing knowledge? – Absolutely, yes. As the authors note several times, comprehensive work of this type, which considers elemental composition, botany, nutrient influx and impacts on plant productivity and Hg quantities and speciation, are not represented in the scientific literature, particularly at the scale they propose.** **Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? – Yes. The conceptual model is very well presented, including a comprehensive discussion supported by data from other sites in the region and figures that assist in making the complex inter–relations of plant physiology, sediment and aqueous geochemistry and microbial processes clear. It does explain the underlying basis for the proposed work.** **Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified? – The selection of this proposed work as a research project is justified.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**In turn;**

	Is the approach well designed and appropriate for meeting the objectives of the project? – Overall, the approach is very ambitious in its scope, but is comprehensive and well designed. For the most part, the stated approach is more than appropriate for meeting the needs of the project. One point that is not made clearly enough has to do with the sampling of sediments. The rhizospheres of plants, regardless of species, exist in both the oxic and anoxic regions of the sediment profile. The authors do not specifically address in detail how sediments will be sampled. Will both oxic and anoxic sediments be sampled? If so, how will the geochemical gradient be preserved until detailed analyses? I would have liked to see this component explained in the same detail as the rest of the approach, since the characterization of the sediment and its geochemistry is an essential component of the proposed research. **Is the approach feasible? – Overall, yes.** **Are results likely to add to the base of knowledge? – This work is ambitious to be sure, but it is also comprehensive in terms of its experimental design and planned approach. I am confident that this work will contribute meaningfully to the base of knowledge.** **Is the project likely to generate novel information, methodology, or approaches? – Novel information, certainly, methodology, perhaps and approaches, perhaps.** **Will the information ultimately be useful to decision makers? – Provided that the authors can present their findings in terms of the primary objectives of the decision makers, yes. This proposal, while be scientifically well written, also does a good job of expaining terms (for the most part) and in relating the objectives of this proposed work to the primary needs of the CALFED program.**
Rating	

	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**In turn;** **Is the approach fully documented and technically feasible? – With the exception of the sediment sampling question raised in the previous section, absolutely, yes.** **What is the likelihood of success? – Overall, the likelihood of success is high.** **Is the scale of the project consistent with the objectives and within the grasp of the authors? – As previously mentioned, the objectives of the project are ambitious and thus require a comprehensive, large and detailed approach, which is provided here. Based on this consideration and the combined record of the authors, my opinion is yes, this work is within the grasp of the authors.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**In turn;** **If applicable, is monitoring appropriately designed? – Yes, absolutely. The authors explain in detail the experimental design in terms of experimental versus control sites, amended versus natural, how each will**

	be sampled and in accordance with what variables (seasonality, annual, etc.). **Are there plans to interpret monitoring data or otherwise develop information? – Yes, plans are stated to interpret monitoring data in terms of the projects stated objectives.**
Rating	**excellent**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**In turn;** **Are products of value likely from the project? – Yes. The authors outline what types of products they anticipate being developed, including publications, USGS fact sheets, CALFED technical memorandums and digital databases.** **Are contributions to larger data management systems relevant and considered? – Yes. The authors discuss the relevance of the proposed work in terms of larger, overall objectives of the CALFED program.** **Are interpretive outcomes likely from the project? – Yes.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**In turn;** **What is the track record of the authors in terms of past performance? – The combined track record of the authors is very good.** **Is the project team qualified to efficiently and effectively implement the proposed project? – Yes.** **Do they have available the infrastructure and other aspects of support necessary to accomplish the project? – Yes.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**– Yes.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**– My summary rating is determined as the average of all categorical ratings.**
Rating	**excellent**

Technical Review #3

proposal title: The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The stated objectives of the proposed work are to improve the understanding of: the spatial and seasonal cycling of Hg in vegetated marshes; how key wetland plant properties influence Hg cycling, and; how nutrients influence plant productivity and physiology, and thus, Hg cycling in wetlands .** **These objectives are clearly articulated in the context of the priorities of the CBDA Science program, and in the context of the implications of CALFED-sponsored wetland restoration efforts.** **These objectives are clearly carried through in Section 2. Description, where they are well articulated as three hypotheses focussing on the distinction between vegetated and non-vegetated sites, nutrient status, seasonality and ecosystem structure.** **The ideas are of urgent importance to the San Francisco Bay Area, and to wetland and mercury science in general, as very little is known about the functional role of wetland vegetation in regulating the mercury cycle, and in particular, methylmercury (MeHg) production. The relationship to other target areas funded by CALFED and other agencies are explicitly outlined on pages 18-20 of the proposal.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is highly justified, relative to existing knowledge. There is virtually no information about the role of wetland plants in the geochemical cycling of mercury in sediments. There is very limited information that suggests that plant assemblages influence Hg speciation, and that certain species facilitate sediment−air exchange, but this information is at best, non−process oriented, and at worst, circumstantial. This research is of particular importance when placed in the context of wetland restoration, where restoration activities themselves may influence Hg speciation. All of this is extremely well documented and well cited within the body of the proposal.** **The conceptual model is presented in the text of the proposal but elegantly summarized in Figures 2 and 4. The lead investigator has been a proponent recently in professional conferences of the role of the oxidized rhizosphere in influencing Hg cycling through the intensification of the sulfur−cycle, and the release of dissolved organic matter that increase microbial activity. This is a new idea that if fully tested, will make significant contributions to science and SF Bay research. This is a very sound model that has the added virtue of being highly testable in an experimental framework. The approach proposed here justifies the support of the full−scale implementation of the work plan.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The overall approach of the proposed research is experimental. Generally using a Before−After−Control−Impact (BACI) approach, the proposed work seeks to address a fairly comprehensive set of questions.** **The use of delineated experimental plots in each of the study wetlands is a highly appropriate methodology that permits in situ manipulation with replication, while controlling for between−wetland variability.** **TASK 6 − Mercury transformations and Speciation in the Rhizosphere.** **The approaches presented in this section are established in the literature, and as such are perfectly acceptable. Standard Hg and MeHg determinations are still challenging, but this research group has demonstrated competence. The determination of reactive inorganic mercury is indeed a fairly standard method, however it is somewhat contentious to equate this fraction with bioavailable Hg. The use of Hg radioisotopes for methylation and demethyation assays has decreased in recent years in favour of stable isotope approaches, mainly because the latter can be undertaken with ICP−MS. Neither method is necessarily superior, so long as high−specific activity radioisotopes are used. The USGS researchers on this project have an extensive track−record with using radioisotopes and as such, may be expected to deliver sound, intercomparable results with other work.**

The research also proposes to couple sulfate and iron reduction rates and other ancillary chemistry with the measures of Hg transformation, which will provide additional insight into the role of the rhizosphere on controlling redox reactions.

TASK 7 – Wetland Plant Structure...

Task 7a–c will provide appropriate physiological and structural measures that will significantly expand the power of the sediment–rhizosphere geochemical observations, and are completely novel contributions in the context of sediment Hg cycling.

Task 7d – Although I am not sure if the wiping technique combined with dry–deposition measurement (contentious!) will yield sufficiently different results to permit a clear observation of process, it is a highly novel approach that warrants testing. The importance of leaf–surface excretion in salt–adapted plants may be an important mechanism of Hg mobilization.

Task 7e – Gaseous Hg evasion. There is some precedent in the literature for this work, which presents methodological challenges, but the authors have fully utilized this previous work. Their suggestions for the testing of experimental apparatus and other controls shows experimental rigour.

TASK 8 – Rhizosphere nutrient biogeochemistry and microbial ecology. The use of N and C isotopes to evaluate plant function is sophisticated. The pore water and sediment nutrient studies provide an additional mechanistic dimension to the overall picture of plant–sediment–chemical interactions. THe exoenzyme measurement scheme is novel in the context of Hg chemistry and methylation.

Finally, coupling this to ecosystem productivity makes a direct connection to restoration strategy, and as

	such is very useful to decision makers.
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**As indicated above, the approaches that are described are completely feasible. The likelihood of success is very high as the project is framed as a series of testable hypotheses. The outcome of the research is highly informative, regardless of the direction of the results. The scale of the project is perfectly targeted to ensure project manageability and feasibility. The substantial experience of the project team is evident in their CVs and research agendas, contributing to a successful outcome. Methods are fully explained in Table 2.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**As indicated above, the Before−After−Control−Impact framework is ideally suited to evaluating outcomes. The mesocosm approach affords replication, experimental treatment and statistical power in the explanations. The scientific data produced will be highly interpretable to allow for mechanistic understanding of the plant−sediment controls on Hg cycling.**
Rating	**excellent**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The products of value will be peer-reviewed publications, agency publications, fact sheets and public workshops. These are all high value outcomes. The high productivity of the authors suggests similarly high output on this funded project. The insights into the role of wetland plants in the Hg cycle will inform the larger efforts underway supporting wetland restoration in the Bay area. The results will be highly interpretable, and the relationship to other target areas funded by CALFED and other agencies are explicitly outlined on pages 18-20 of the proposal.**
Rating	**excellent**

Additional Comments

Comments	**This is one of the most thoroughly researched, substantiated and well composed research proposals that I have ever had the pleasure of reading.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The authors are all highly capable of undertaking the research. Marvin-Dipasquale is an established and respected mercury scientist who is a leader in the area of methylation/demethyation processes, and a pioneer in the area of wetland-plant-Hg interactions. Windham has extensive expertise in wetland plant ecology. Capone and Jacobson both have a high level of demonstrated expertise in the areas of nutrient**

	biogeochemistry, and Carpenter is an expert in the area of ecosystem productivity. All have strong to extensive publication records, and strong to substantial external funding records. **The combined analytical research capabilities and infrastructure managed by all of the authors is impressive. There are no limitations to the project in this respect.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The majority of the cost of this project is the salary of the investigators and support staff. I can only surmise that these amounts are appropriate and adequately documented as I am not entirely familiar with the way in which external funds contribute to their base salaries in the varios agencies. The costs for the experimental and analytical work are relatively low, and in line with reasonable operating expenses over three years. The more substantive infrastructure is already in place with each of the principal investigators. The outcome to cost ratio for this project is very high.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is, without exaggeration, the most well composed research proposal that I have ever evaluated. The proposal is thorough, elegantly**

	assembled and without technical errors. **The problem foci, methodologies and research team composition are perfectly executed. All research questions are fully substantiated by the thorough literature review.** **The research is highly mechanistic and is well positioned within the broader CALFED efforts of larger watershed scale studies, modelling and more estuarine research. The specific processes that will be elucidated by this research will contributed specifically to a much deeper understanding of Hg cycling in the Bay Area, and to the field of Hg science more generally.** **Many research projects are either well substantiated and lack innovation, or are innovative but lack a clear approach that engenders confidence of outcome. My rating is excellent because of the high level of innovation coupled with the expertise and clear approach to generate a successful outcome.**
Rating	**excellent**

Technical Review #4

proposal title: The Role of Wetland Plants and Nutrient Dynamics on Mercury Cycling: Identifying Critical Processes for Wetland Restoration in the San Francisco Bay

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Goals, objectives, and hypotheses are very clearly stated. Significance of MMHg production to ecosystem health and eventually to human health issues suggests that interdisciplinary studies on Hg cycling need to be completed as soon as possible.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The research proposal is strongly justified by the authors, and a clear conceptual model is provided to explain the hypothesized interrelationships between microbial, plant, and biogeochemical processes. The scale of the study and the linkages among the different components of this study also are well explained and justified.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Yes, the approach is well designed and appropriate. However, the lack of consideration to the hydrologic regime, which could impose the most significant control on MMHg dynamics, may be "veiled" by the conceptual framework and proposed sampling scheme presented in this proposal.** **The proposed interdisciplinary research, evaluating ecosystem processes on nutrient and contaminant cycling, is timely and would contribute novel information that should be publishable in peer-reviewed literature.** **The information may be useful to decision makers; however I am leery of the possible conclusion or generalization that wetland restoration enhances MMHg production, especially without careful consideration to hydrologic influences.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Ambitious project, but research team has strong experience and demonstrated success, indicating a strong potential to complete the proposed tasks.** **Comparing the plots within a hydrologic framework (currently not considered) would improve the likelihood of success greatly.**

Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The proposed 3−year timeframe for the study may not be long enough to evaluate before/after treatments, especially if the methods are not yet defined (e.g., method, amount, and timing of fertilizer application).** **Although 20 plots are proposed, the various treatments lead to small sample sizes (n=3) hat may limit the researchers' capability of producing conclusive or reliable results. This is compounded by the strong potential for hydrological influences to impose more significant effects on the identified response variables.** **Will biomass harvests interfere or disturb study sites?**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The sampling scheme may provide inconclusive or even misleading results, because of the small sample sizes and the different locations of the two sites (Coon Island and Pond 2A). Hydrologic controls, rather than plant−induced effects, likely exert more influence on MMHg dynamics and explain differences among the two sites. However, the lack of hydrologic data will**

	preclude the researchers from considering this influence. **The proposed sampling scheme seems to assume that each wetland site is a homogenous unit, and does not address how hydrology might influence the distribution and arrangement of plant species and influence the results.**
Rating	**good**

Additional Comments

Comments	**If temperature, pH, labile C availability, and redox conditions (especially in terms of the supply of terminal electron acceptors) primarily influence microbial processes and hence MMHg cycling, then it seems likely that distinct spatial patterns within a wetland complex and between sites arise from variation in hydrology rather than the influence of vegetation. The fact that the proposed target plant species occur in distinct assemblages suggests the importance of an environmental or abiotic spatial gradient. Further, across a flat wetland, we could expect that a 1–2 feet difference in land surface elevation would affect the interaction of different water sources significantly enough to alter biogeochemical processes. The current study design could "miss" the true ecosystem driver (i.e., plant effects could be superimposed on the hydrologic mechanisms driving ecosystem processes). This proposal would be outstanding if the sampling plan was developed more in a hydrologic context, with consideration to ground–water flow patterns.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Strong credentials and resources to complete the**

	project.
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Most of the budget request would support salary and benefits. The breadth and intensity of the project probably will require the hours requested.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**Marvin-DiPasquale et al. present a well cited and well written proposal to study a timely issue that is very relevant to the environment and human health. The broad, inter-disciplinary approach will require intensive efforts from a number of experts in different fields, leading to a rather expensive research investment.** **The lack of consideration to hydrological influences, especially surface and ground-water interactions, could limit the potential for success of this project.**
Rating	**very good**